

THE AMERICAN PSYCHOLOGIST

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PSYCHOLOGY

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

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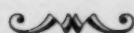
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THE LOGICAL BASIS OF FACTOR ANALYSIS

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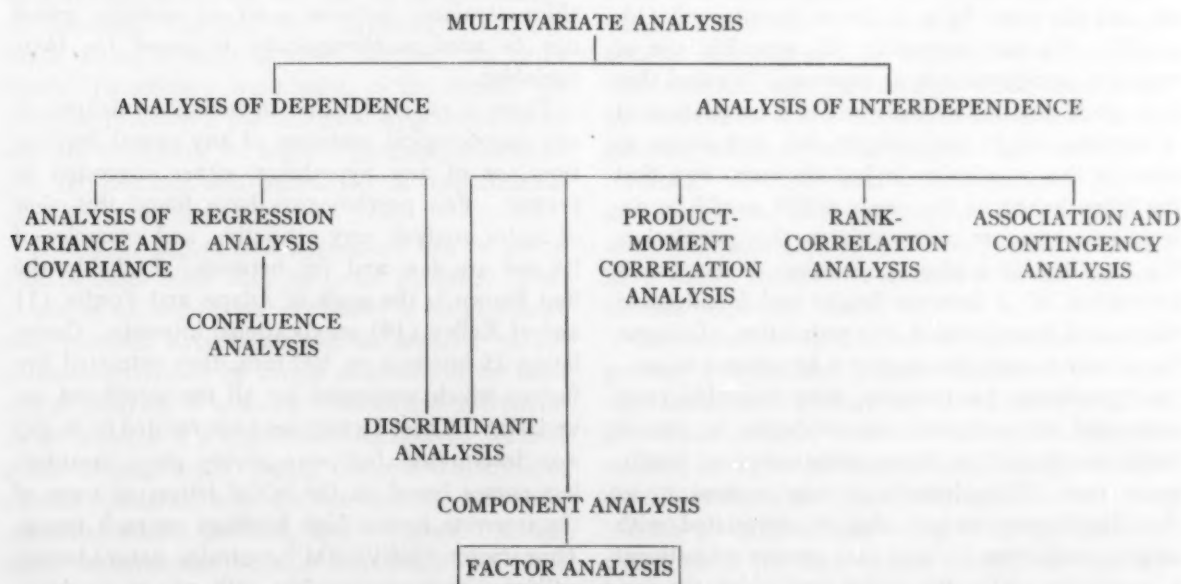
FEW methods of statistical analysis have encountered as much resistance among both statisticians and psychologists as has factor analysis. In addition to this critical attitude towards factor analysis as a whole, there is an internecine quarrel amongst practitioners which has split the whole field into schools, factions, and individual snipers. Is this turmoil due to any inherent flaws in the method, or is it due to some confusion about aims and techniques? It will be the burden of this paper to show that the latter possibility is the more likely cause, and to attempt the construction of a rational scheme into which all the existing methods of analysis can be fitted. The stress is throughout on the *logic* as opposed to the *mathematics* of factor analysis; disputes about the latter are much less fundamental and much more easily settled than discussions about the former.

In assigning a place to factor analysis in the general field of statistics, we may with advantage follow Kendall (12, p. 60), who draws a distinction between analysis of *dependence* and analysis of *interdependence*.

In the latter we are interested in how a group of variates are related among themselves, no one being marked out by the conditions of the problem as of greater prior importance than the others, whereas in the analysis of dependence we are interested in how a certain specified group (the dependent variates) depend on the others. The distinction is perhaps seen at its simplest in the bivariate case: correlation between two variates is a matter of interdependence, and is a symmetrical relationship between them; the regression of one on the other is a matter of dependence and is not a symmetrical relationship—the regression of x on y is not the same as the regression of y on x .

The position of factor analysis in the group of techniques using analysis of interdependence is shown in the accompanying figure quoted from Kendall (12, p. 61).

I have said: "*The position of factor analysis*," but the use of this clause suggests erroneously that there is one technique, one method, and one aim underlying the quite variegated activities of factor analysts. In actual fact, there are three main aims which factor analysts try to achieve, three main views regarding the nature of factors which are closely related to these aims, and a large variety of methods of extraction and techniques of rotation.



If we would understand these many different approaches, we must start with a statement of the questions which factor analysis is trying to answer. This is particularly important because of the tendency of many critics to reject the factorial answer to a certain question, not because the answer is inadequate, but because the question is misunderstood.

This point is well made, in quite another connection, by the philosopher Collingwood who writes:

You cannot tell whether a proposition is "true" or "false" until you know what question it was intended to answer . . . a proposition which in fact is "true" can always be thought "false" by any one who takes the trouble to ex-cogitate a question to which it would have been the wrong answer, and convinces himself that this was the question it was meant to answer. And a proposition which in fact is significant can always be thought meaningless by any one who convinces himself that it was intended as an answer to a question which, if it had really been intended to answer it, it would not have answered at all, either rightly or wrongly (5, p. 30).

The three aims of factor analysis are the same three aims which give rise to other branches of statistics. As Kelley (13, p. 22) puts it: "The first function of statistics is to be purely descriptive, and its second function is to enable analysis in harmony with hypothesis, and its third function to suggest by the force of its virgin data analyses not earlier thought of." Kelley makes clearer his second and third points by adding: "We may say that there are two occasions for resort to statistical procedures, the one dominated by a desire to prove a hypothesis, and the other by a desire to invent one" (13, p. 12). We may exemplify this threefold use of statistics by reference to an example. We find that in a given population there exists a correlation of .6 between height and weight; this fact serves to *describe* this population in just the same way that the mean height or the mean weight would be descriptive constants characterizing this population. We find that in a given population there exists a correlation of .2 between height and intelligence; this is also descriptive of this population, of course, but it may in addition suggest a hypothesis to us—the hypothesis, for instance, that favorable environmental circumstances are conducive to greater bodily height and to better performance on intelligence tests. This hypothesis may suggest to us that intelligence should also be correlated with weight (deduction 1), and that greater educational homogeneity within the group over which the cor-

relation between height and intelligence is being run should reduce that correlation (deduction 2). We therefore calculate further correlations to prove or disprove our hypothesis—disprove in this case, because both deductions are falsified by the facts—thus using statistics to carry out "analysis in harmony with hypothesis."

Most psychologists and statisticians are aware, either explicitly or more frequently implicitly, of these three uses of statistics; it is in their application to factor analysis that problems of communication arise. I shall therefore discuss the use of factor analysis at these three levels in some detail, giving a formal definition of the term "factor" appropriate to each level.

Factors as descriptive statistics. Whatever else may be the function of a factor, it is always descriptive of a given sample or population. It is small wonder, therefore, that many definitions of factor analysis stress this point to the exclusion of any other. Thus Holzinger and Harman (11, p. 1) write: "Factor analysis is a branch of statistical theory concerned with the resolution of a set of descriptive variables in terms of a small number of categories or factors. . . . The chief aim is . . . to attain scientific parsimony or economy of description." Similarly Kelley (15, p. 120): "[Factor analysis] represents a simple straightforward problem of description in several dimensions of a definite group functioning in definite manners." We may thus arrive at the following definition: *A factor is a condensed statement of (linear) relationships obtaining between a set of variables which can be used mathematically to stand for these variables.*

There is no implication in factors so defined of any psychological meaning, of any causal implications, or of any hypotheses, either suggested or proved. Few psychologists have found this view of factor analysis very attractive, and examples of its use are few and far between. Probably the best known is the work of Adams and Fowler (1) and of Kelley (14) on vocational interests. Correlating 35 interests on 800 men, they extracted five factors which accounted for all the significant covariation. These factors were not rotated or in any way interpreted, but were simply given meaningless names based on the initial letters of some of the interests having high loadings on each factor. Thus factor "NEVCOM" contrasts nature-loving, religious, and salesmanship with power, mechani-

cal, spatial, orderliness, verbal, and musical interests. The fact that psychologically these factors make no sense would not be a correct criticism of the analysis which was intended to be purely descriptive; descriptively these nonsense factors are as good as any other set. The obvious convenience of having five factors instead of 35 variables in a regression equation, for instance, is undoubted, and may furnish justification for this very limited type of analysis. (We would of course lose whatever contributions specific factors might have made, possibly a very serious loss indeed.)

Factors suggesting a hypothesis. A factor, however it may have been obtained, may suggest a hypothesis to the investigator. In so far as it does that, the factor ceases to be merely descriptive and becomes part of theoretical psychology. We may give a formal definition of factor analysis from this point of view and say: *A factor is a condensed statement of (linear) relationships obtaining between a set of variables, suggestive of hitherto undiscovered causal relationships.*

As an example of this type of approach, contrasting with Kelley's analysis mentioned above, we may cite Thurstone's (22) analysis of the intercorrelations between 18 of Strong's interest scales. Thurstone emerged with four factors, labelled interest in science, interest in language, interest in people, and interest in business. Later work, summarized elsewhere (9), has on the whole confirmed Thurstone's analysis, and there is little doubt that the hypothesis suggested to him by the analysis has psychological meaning, and fits into psychological theory, while Kelley's analysis does neither. In other words, Thurstone's work adds to the merely descriptive function a psychological hypothesis which appears reasonable on a priori grounds, and which can be checked and submitted to experimental verification or disproof.

It may be argued that hypotheses can be generated in other ways than by factor analysis. That of course is undeniable, and there is no guarantee that factorial hypotheses will be superior to hypotheses arrived at by simple observation, by theoretical analysis, or even by *Schreibtschexperiment*. As an example of a hypothesis thus derived, we may quote Spranger's (20) purely theoretical analysis of interest patterns into the theoretical, economic, aesthetic, social, political, and religious. A factorial investigation of this hypothesis by Lurie (16) gave rise to four factors: social, theoretical,

religious, and what he calls Philistine, i.e., combining the economic and political interests, and opposing them to the aesthetic interests. This investigation thus gives results in line both with Thurstone's and with Spranger's hypotheses, but as Lurie's work is essentially an example of factor analysis supporting or disproving a hypothesis, no more will be said about it.

While it must be admitted that hypotheses may be formed in a variety of ways, the factorial method has one definite advantage. It provides *ab initio* data relevant to the formation of such hypotheses, and it rules out a large number of possible hypotheses which might otherwise have been entertained. Something similar, of course, is done when hypotheses are based on a simple observational study; indeed, it will be argued later on that much observational and clinical work is essentially similar to factor analysis in principle, though inferior to it because of its lower degree of rigor and accuracy. In a well-studied field, there are probably enough well-documented observations to make hypothesis-formation easy; in a relatively new field, the help of factor-analytic methods may be very important in accelerating the formation of reasonable, worthwhile hypotheses, and in discarding poor ones.

Factors supporting or disproving a hypothesis. It is obvious that factor analysis cannot be used as a formal part of the hypothetico-deductive process in relation to just any type of hypothesis. The great majority of psychological hypotheses require some form of analysis of dependence, and thus rule out the factor-analytic approach. But there are a number of hypotheses, particularly those concerned with structure and organization, which require factor-analytic methods, and which are difficult at the moment to disprove or support by nonfactorial methods. All type- and trait-hypotheses, for example, fall into this category, as I have tried to show elsewhere (6, 9), and even Freudian theories have shown themselves amenable to the factorial method of proof (10).

Our definition of a factor as supporting or disproving a hypothesis follows directly from these considerations. We may say that *a factor is a condensed statement of (linear) relations obtaining between a set of variables which is in agreement with prediction based on theoretical analysis.*

Such predictions may be of varying degrees of exactitude. We may predict merely that certain items or tests will be found to have positive projec-

tions on a factor, while other items or tests will have negative projections; this is the most elementary level of prediction. Much more refined prediction is possible in relatively well-studied fields. Thurstone would probably be able to specify with considerable precision the position of a newly constructed test in the cognitive multi-factor space, or to construct a new test to specification, i.e., to lie at a particular place in the multi-factor space. The writer has been able to write social-attitude items to specification within a defined two-factor space with negligible errors. Many other examples of relatively precise predictions could be given; the majority of cases, however, would undoubtedly be at a much lower level of precision.

Frequently psychologists fail to state the exact nature of their hypotheses, and discuss their findings as if they had selected their tests at random, without any kind of hypothesis in mind. Occasionally such blind empiricism does seem to lie at the back of factorial work; factor analysis is sometimes used as a last resort to try and rescue worthless data accumulated at random from the fate such data so richly deserve. It need hardly be said that such use of factor analysis is valueless, but the fact that it occurs should not be used as an argument against the method as such; similar faulty use may be the fate of all statistical methods. It is probably safe to say that in the great majority of cases items and tests are included in a factor analysis on the basis of fairly specific hypotheses which are seldom verbalized in the write-up of the experiment because (a) lengthy discussions would be required, which most editors would refuse to print, and (b) results are already available to show which hypotheses have been verified, so that there seems little point in discussing those which have been disproved.

It will have been noticed that in passing from the purely descriptive use, there has been a definite change in the implication of the term *factor*. For Kelley, there is no causal reference implied in a factor; for Spearman, Thurstone, and those who follow their methodology there quite clearly is such a reference. This causal implication characterizes not only the interpretation of factors as suggestive of a hypothesis, but also the next level of factors as proving a hypothesis, and since from the psychological point of view this causal implication is precisely what lends interest and value to factor analysis, it may be opportune here to give a definition

of a factor which brings out this element. We may therefore offer the following definition: *A factor is a hypothetical causal influence underlying and determining the observed relationships between a set of variables.*

This definition serves a useful function in drawing attention to the close link between the hypothesis-generating and the hypothesis-proving functions of factor analysis, as opposed to the purely descriptive. It will often be found that in one and the same investigation there will be factors which support a hypothesis and factors which generate one. Elsewhere (6) a factor analysis has been reported of a large matrix of neuroticism tests designed to test a hypothesis regarding this particular factor. The analysis did indeed confirm the hypothesis; it also gave rise, however, to another factor which suggested that pencil-and-paper, verbal-type tests are separated in a clear-cut fashion from nonverbal, objective-behavior tests. Such a verbal-nonverbal factor, well known in the cognitive field, suggests various hypotheses which require testing; thus we find in the same analysis confirmation of one hypothesis, and suggestions for further hypotheses. This mutual stimulation between proof and suggestion might indeed be regarded as a prominent feature of factor-analytic work, and may recommend the method to those used to this interplay among hypothetico-deductive lines in other sciences.

While this interplay of proof and suggestion is valuable and important, it has often led to interpretations highly vulnerable to criticism. As has been pointed out elsewhere (7), it has been one of the worst abuses of factor analysis that practitioners have often carried out an analysis *suggesting* a hypothesis, and have then gone on to argue that their analysis has *proved* this hypothesis. The distinction is fundamental, and much of the criticism often made of factor analysis is ultimately referable to this failure to be clear about the status of the factors isolated.

We have discussed so far the *aims* of factor analysis; we must now turn to the *nature* of the factors isolated. A factor may be regarded as a purely statistical concept, an "artifact" if you like, akin to an average, a variance, or an epsilon. This view of the nature of a factor clearly corresponds with the descriptive aim as outlined above. As such, the concept is clear and does not require further discussion.

Secondly, a factor may be regarded as a principle

of classification. This is the view of Burt (3), who likens factors to lines of longitude and latitude. According to this view, factor analysis first removes whatever is common to all the tests or items correlated, and then proceeds by means of a series of bipolar factors to disclose the principal ways of classifying the material under discussion. This is done without rotation of axes, it being assumed apparently that the principles of classification would remain invariant under change of tests or items correlated. This assumption is almost certainly mistaken in the majority of cases, but does not seem to be an indispensable part of this view of the nature of factors.

The third way of looking at factors is to regard them "as if" they were causal agencies. This view is implied in the definition given of the term "factor" a little while ago; it is given clear expression by Thurstone (22, p. 54), who writes: "One of the simplest ways in which a class of phenomena can be comprehended in terms of a limited number of concepts is probably that in which a linear attribute of an event is expressed as a linear function of primary causes." Spearman's (19, p. 75) view is similar: "... if meaningful as opposed to statistical, a factor is taken to be one of the circumstances, facts, or influences which produce a result."

There are two main criticisms of this view. Some writers hold the view that even if such causes could be identified in mental life, factor analysis could still not be relied upon to identify and isolate them. Thus Kelley, continuing the quotation in which he defined factors as being purely descriptive, says: "... he who assumes to read more remote verities into the factorial outcome is certainly doomed to disappointment" (13, p. 22). Burt (3, p. 231) objects to the use of causal terms on philosophical grounds; he admits, however, that in certain cases "the language of causation is not only convenient, it is almost unavoidable, if we are to remain comprehensible." His main objection appears to be not to the language of causation as such, but rather to the reification of factors.

It is at this point that we encounter the central problem in our quest for the logical basis of factor analysis. It is here, also, that most critics have claimed to find the most vulnerable spot in the armour of factor analysts. If a given factorial solution is "purely arbitrary," just one of an innumerable number of possible solutions, and if it carries no causal implications, then it appears to many

critics to differ fundamentally from other types of mathematical solutions, and to give rise to concepts much more insecurely based than those in other sciences.

This type of criticism appears to be based on a profound misunderstanding of the nature of scientific laws and concepts. As Thurstone (22, p. 51) points out:

... the constructs in terms of which natural phenomena are comprehended are man-made inventions. To discover a scientific law is merely to discover that a man-made scheme serves to unify, and thereby to simplify, comprehension of a certain class of natural phenomena. A scientific law is not to be thought of as having an independent existence. ... A scientific law is not a part of nature. It is only a way of comprehending nature.

In a sense, therefore, the concepts and laws to which factor analysis gives rise are "statistical artifacts"; they are so in the same way that all other scientific concepts and laws are "artifacts." Spearman's *g* (general intelligence) is a statistical artifact to precisely the same extent, and for the same reasons, that Newton's *g* (gravitational force) was a mathematical artifact. Neither has any actual existence, in the sense that a falling stone or an individual who is acting intelligently can be said to exist; both concepts are abstractions which serve to unify and simplify a complex class of phenomena, and both had to be discarded or amended when new facts showed them to be incapable of accounting for all the phenomena. It does not appear reasonable to criticize factor analysis for showing features which are characteristic of all science.

Nor is the alleged "subjectivity" of factor analysis absent in universally accepted forms of dimensional analysis in physics. The physicist Bridgman (2, p. 1) points out that "there is nothing absolute about dimensions—they may be anything consistent with a set of definitions which agree with experimental fact." And Scott-Blair (18) has given an example of alternative dimensional analyses of the phenomena of heat. We may, therefore, dismiss this criticism also as applying to all science equally, rather than just to factor analysis. All science, in a sense, is an "artifact" and "subjective"; the important point is that this artificiality and subjectivity are closely circumscribed by the need always to remain in accord with the facts science sets out to unify and simplify. Those who have had experience in trying to formulate a hypothesis, whether factorial or otherwise, which would account

for a large number of different phenomena will not usually be worried about having to choose one of a very large number of such theories; they will be thankful indeed if even one theory can be found which is not decisively contradicted by several indisputable facts. Psychologists sometimes tend to overcome this difficulty by disregarding those facts not in accord with their particular theory; there is little in the history of science that would encourage such a policy of neglect.

Granted that the most usual objections to factor analysis and the "causal" status of factors are invalid and based on an imperfect understanding of scientific methodology as a whole, our argument cannot be based entirely on disproof of criticism; it would seem desirable to argue more directly from positive evidence. There are four such proofs. The first proof relates to conditions where the causal relations are relatively well understood, and where we can compare the results of factor analysis with independent knowledge of the conditions responsible for the results. An excellent proof of this nature is supplied by the outstanding work of Wenger (24) on the "autonomic imbalance" factor. Following Eppinger and Hess, whose theory of "sympatheticotonia" postulated a predominance in certain subjects of sympathetic innervation, Wenger gave a battery of tests involving measures of the effects of autonomic innervation to various groups of subjects, including children and normal and neurotic adults, and carried out a Thurstone-type factor analysis of the resulting intercorrelations. Simple structure revealed in each of several analyses a clearly marked factor of "autonomic imbalance," having high saturations in the predicted direction on the predicted variables. Here we have an intelligible "cause" underlying the observed correlations, and the coincidence of factor saturations with theoretically predicted pattern is surely too striking to be ascribed entirely to chance. It may be noted incidentally that until taken up by factor analysts the Eppinger-Hess theory lay dormant, except for theoretical discussion, for some thirty years, because no other statistical-experimental procedure lent itself to the investigation of this type of hypothesis.

The second type of proof relates to the simultaneous change of scores on all the tests defining a factor when the hypothetical physiological basis of that factor is experimentally altered. As an example we may quote the work of Petrie (17) on the after-effects of lobotomy in neurotic subjects.

Basing her work on the hypothesis that patients after operation showed changes on the factors of neuroticism and extraversion-introversion along the lines of *decreasing* amount of neuroticism and *increasing* amount of extraversion, she administered before operation two sets of six tests defining these two factors respectively, and predicted the direction in which change would take place. In all cases tests carried out after operation verified the prediction; in other words, all the tests defining the factor of neuroticism showed changes in the direction of lessened neuroticism, and all the tests defining extraversion showed changes in the direction of increased extraversion. This dynamic proof for the functional unity and biological reality of the factors in question is particularly impressive because of the paucity of statistically significant changes on personality tests previously reported in the literature.

The third method of proof is based on the following argument. It is possible to calculate an approximate index of hereditary determination, such as Holzinger's h^2 , for any test which has been applied to a sufficiently large sample of identical and fraternal twins. It is difficult to see how a factor, which is merely a linear combination of test scores, can have a higher h^2 than any of the constituent tests, unless this factor is based on some very definite, underlying biological reality or function which is itself inherited. There is at least one study (6) in which it has been shown that the factor of neuroticism has a higher h^2 (.810) than any of the constituent tests which are combined to give that factor score; the highest individual test h^2 was .701. It seems difficult to dismiss as "subjective" and as a "statistical artifact" a factor having such very definite and obvious relation to biological reality.

The last method of proof suggested here is more indirect than the others, but logically equally important. Factor analysis is often considered to be a complete innovation, something different from, and possibly even contrary to, the usual methods of scientific investigation. It is the burden of this paper to point out that quite on the contrary methods logically identical with factor analysis, though mathematically less exact and rigorous, have been used from the very dawn of science to deal with the type of problem involved in the study of "interdependence." In doing so, they have led on to hypotheses regarding "causes" and to analyses of "dependence" which have greatly clarified the field,

and which would have been impossible without the preceding "factorial" investigations. As this point is crucial to my argument, I shall give two examples of what I have in mind.

Let us take first of all the concept of disease. If we take a particular disease, such as tuberculosis, we know now that it is caused by an identifiable "cause," namely the tubercle bacillus, acting on a human body which may vary from case to case in its resistant properties. This particular disease, however, was known and isolated long before the "cause," the bacillus, was discovered; indeed, unless the disease had already been known as a unitary entity it is difficult to see how its "cause" could have been discovered. How, then, was the disease identified? It was identified essentially in factorial terms, i.e., by the fact that certain physical symptoms—loss of weight, breathing difficulties, high temperature, coughing-up of blood, etc.—tended to go together (intercorrelate) in a certain manner as a "syndrome." No symptom by itself is decisive (none is factorially pure), but the syndrome (factor) suggests one underlying cause which gives rise to the various symptoms, and which may sometime be identified. In a similar way, we identify mental diseases in what is essentially a factorial manner, i.e., in terms of the observed intercorrelation of various symptoms; anyone reading Kraepelin or Bleuler will be able to follow this process in its clearest and most obvious manner. In the mental field we have not yet discovered the underlying cause of the various patterns we observe; until we do we have to rest content, as we had to in the case of physical medicine before the advent of Pasteur, with syndromes (factors). All that factor analysis does is to make explicit and rigorous what the clinician does in any case, often implicitly and without full understanding of his methodology. Both the clinician and the factor analyst may be mistaken, and group together what does not in fact (causally) belong together; medical history indicates a number of errors as well as a remarkable number of successes in this preliminary method of grouping together symptoms in terms of underlying "diseases." It seems reasonable to assume that greater rigor and awareness of the pitfalls involved may decrease the number of errors; there is no way of guaranteeing complete success. The point to stress is that this "factorial" stage is an indispensable preliminary to the "causal" stage; our factor or syndrome tells us what symp-

toms go together in such a way that we can with some hope of success go on to look for a single underlying cause.

My second example relates to the field of taxonomy in flora (21) and fauna (4). Until the advent of Darwin and the theory of evolution, the only way of telling "what goes with what" in the plant and animal kingdoms was by means of morphology, i.e., by noting degrees of similarity of a large number of outwardly observable characteristics. Thus, specimens agreeing on a large percentage of such characteristics (correlating highly together) were considered to be closely related; specimens agreeing on a small percentage of characteristics only (correlating together at a low level) were considered to be only remotely related. By means of an implicit and nonrigorous factor analysis of these similarities or correlations the whole elaborate system of 19th century systematics was built up. The theory of evolution made it possible to check the resulting taxonomic picture with its implied causal influences against the directly observable causal development shown by Darwinian research. The remarkable result was that in its main outline the picture required very little change; there were many details which had to be modified, but by and large morphology had been an extremely accurate guide to causal relations (4, 20). So here also we find that subsequent causal "analysis of dependence" verifies in considerable detail the results of "analysis of interdependence" carried out along essentially factorial lines. And again, the advances made by Darwin would not have been possible without the spade work of the "systematists" and their implicit factorial approach.¹

¹ It might be pointed out that factorial logic plays a part even in such apparently remote fields as in the definition of a metal. A metal is electropositive, forms metallic crystals, its halides generally form ionic aggregates and are non-volatile, but give conducting solutions in water, and its oxides are usually basic. There are, however, exceptions to these rules. Thus SnCl_4 is a volatile liquid; ZnO and Al_2O_3 are amphoteric, and some higher oxides such as CrO_3 are acid. Graphite, arsenic, and tellurium, on the other hand, exhibit metallic properties, while counted among the non-metals. The concept "metal," therefore, rests on the intercorrelation of the various indices enumerated; these correlations are far from perfect, and the only reason for using the term "metal" is the logical implication of a fundamental common feature which unites all metals in a group, and sets them off in comparison with the other elements. This may be an unusual way of looking at chemical concepts, but the logical similarity of derivation is too striking to be passed over.

It may seem fanciful to regard these time-honored methods of analysis as similar in essence to modern factor analysis; yet it would be difficult to deny the essential identity between past and present as long as we consider the logical basis of the procedures involved. It is widely recognized that the correlation coefficient is merely the statistical expression of what Mill called the "method of concomitant variation," and Mill's fifth canon—"Whatever phenomenon varies in any manner, whenever another phenomenon varies in some particular manner, is either a cause or an effect of that phenomenon, or is connected with it through some fact of causation"—certainly preceded in time the statistical superstructure erected on this logical foundation. It is difficult, therefore, to see why a similar process of growth from logical implication and nonrigorous use to statistical elaboration and explicit formulation should not have taken place with respect to factor analysis.

We may conclude, then, that there is both direct and indirect evidence that factorial procedures may lead to genuine causal determinants. It would not be reasonable to say that such a happy outcome would inevitably attend the application of factor analysis; under certain circumstances it may be predicted with confidence that no causal hypotheses will be suggested or proved by factorial methods. In part the outcome of a factorial investigation is determined, of course, by such imponderable factors as the sagacity of the investigator, his skill in framing hypotheses and constructing tests, and his desire to use factor analysis as a hypothetico-deductive tool, rather than as a purely descriptive method. But in part there is no doubt that the actual method of analysis itself will determine the outcome.

At the purely descriptive level, there is little to choose between the methods of analysis advocated by Hotelling, Tryon, Thurstone, or Kelley; the slight advantages of "principal components" are offset by the greater ease of computation of "centroids," and so forth. It is when we come to the problem of rotation that the crucial step occurs. It is clear that we cannot expect a factor to be related in any direct manner to a hypothetical cause unless the factor is unique and invariant in its derivation. Unrotated factors of any kind are usually neither unique nor invariant. This problem is disregarded by writers like Kelley and Burt because the aim for which they use factor analysis is not that of isolating causal determinants. It would

appear, however, that Burt at least is not justified in regarding unrotated factors as giving rise to stable "principles of classification" any more than they give rise to causal determinants; they may occasionally bear a superficial resemblance to such more stable factors but logically a Burtian solution is at the same level of pure *ad hoc*, elementary description as is Kelley's.

If we wish, then, to obtain factors which are not ruled out *ab initio* from fitting into a general descriptive-causal scheme because of lack of uniqueness and invariance, rotation becomes necessary. Here the only scheme which deserves serious consideration is Thurstone's suggestion of rotation into "simple structure," with its attendant concepts of "oblique factors" and "second-order factors." I have on occasion been somewhat critical of Thurstone's earlier work, and it is only right to say here that his recent amplification and development of the more rigid framework of "Vectors of the Mind," together with experimentation of my own, have led me to a reversal of this attitude, into almost complete agreement with Thurstone's latest position. Logically, his method of rotation and experimentation generally amount to this. If we can treat our test domain as if its communality were due to a small number of isolable causes, then our best way of isolating and measuring these causes is by purification, i.e., by selection of tests whose variance is due, not to all these causes at once, but only to one or at most two. This should give us clear-cut differentiation and separation of factors; at the same time, the fact that such selection is practicable provides an impressive proof for the usefulness of the original assumption. Logically this argument seems faultless; mathematically, the scheme has not been worked out to perfection, but there seems to be no difficulty in principle.

In certain practical situations, the full Thurstonian procedure may not be practicable for various reasons, and when we have available an external criterion which embodies a certain hypothesis which we are interested in testing, the method of "criterion analysis" which I have described elsewhere (8) may serve as a substitute. This method appears particularly apposite in personality research outside the cognitive field, for reasons which also have been given elsewhere.

We must now pull together what of necessity has been a somewhat rambling discussion of a large number of related points. This summary can best

be put as a series of numbered propositions; these are not meant to be taken as definitive in any sense, but they may serve to give some orientation to the very discursive criticisms of factor analysis which appear from time to time in the literature, and which are almost wholly concerned with the logic, rather than with the mathematics, of factor analysis.

1. Factor analysis is a mathematical procedure which resolves a set of descriptive variables into a smaller number of categories, components, or factors. These factors themselves, in the first instance, may be regarded as having a purely descriptive function.

2. Under certain circumstances, factors may be regarded as hypothetical causal influences underlying and determining the observed relationships between a set of variables. It is only when regarded in this light that they have interest and significance for psychology.

3. The logical justification for inferring a causal factor from observed correlations is identical with the general scientific justification for inferring causes from effects; more specifically, there is formal identity between factorial procedures in psychology and taxonomic and nosological work in other sciences (medical, botany, zoology).

4. The term "cause," in this context, is a concept which aids in the simplification and unification of natural phenomena; like all scientific concepts it is abstract and consequently an "artifact." A scientific concept is not a part of nature, it is rather a way of comprehending nature.

5. Factors, and the causal determinants which they suggest, are "subjective" in the same sense that physical dimensions are "subjective"; they "may be anything consistent with a set of definitions which agree with experimental facts." Their value and importance arises from the objective reference given them by this agreement "with experimental fact."

6. Criticism of factor analysis as a whole, or of one method of analysis by a writer favoring another method, is often vitiated by (a) lack of historical perspective, (b) lack of scientific sophistication, (c) lack of understanding of the particular problem which the factor analyst is trying to solve. It is usually easy for the critic to invent a problem which the analyst did not try to answer, but to which his answer would have been wrong or nonsensical. This is not a useful form of criticism.

7. The factorial method, no more than any other,

cannot guarantee the correctness of the causal hypotheses suggested by it. Historical evidence reviewed suggests, however, that it is more successful than any alternate method, and that the hypotheses generated by it have proved remarkably accurate when direct experimental test became possible.

8. As indicated above, much nosological work in medicine and psychiatry is essentially of a factorial kind, although lacking the rigor and explicitness of factor analysis. It seems likely that a more formal use of these recent mathematical developments will improve more intuitive "clinical" types of analysis.

9. Methods of statistical analysis, and particularly questions of rotation, are dependent on one's views of the aims of factor analysis, and of the nature of factors; implications of causality require rotation into simple structure, while purely descriptive aims are satisfied equally by nonrotated factors.

10. In the present stage of development of psychology, factor analysis is an indispensable method of taxonomic and nosological research. Knowledge of its historical roots, its logical basis, as well as its statistical methodology, should form part of the training of every psychologist who wishes to understand the standard scientific method of defining concepts in personality research.

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PSYCHOLOGY AS A SCIENCE, AS A TECHNOLOGY AND AS A PROFESSION¹

Address delivered Thursday evening, July 26, 1951 at the 10th International
Congress of Psychotechnology, Gothenburg, Sweden

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YOUR thought is invited this evening to the steady growth of our discipline. This phenomenon is worldwide. In Scandinavia it was relatively late in starting, but is now proceeding apace, as elsewhere.

We are called upon at this Congress to make decisions about international organization and cooperation, as well as about the steps we shall take to encourage a wholesome growth of psychotechnology in the various countries of the world to which we shall presently disperse.

I maintain that the development of psychotechnology is inextricably bound up with the development of psychological science on the one hand, and on the other, with the youthful profession of the practicing psychologist. These three activities of modern psychology—scientific, technological, and professional—must not be severed. Their vitality and strength derive from the same roots. They are organs of one body. Shall we passively permit the powerful centrifugal forces of specialization to rip this organism apart? I hope not.

First, let us be clear as to terms.

Psychology is familiarly defined as a science which aims to describe and to explain behavior. By resort to accurate observation, measurement and experiment, it undertakes to provide the knowledge with which to understand what living organisms do—in particular what people do—and why.

Psychology as a technology has to undertake the still more difficult task of finding ways of predicting and controlling behavior. It is called upon to help in achieving the aims of industry, of education, of mental hygiene, of commerce, of government. In-

deed, psychotechnology might be defined as psychology directed toward aims other than its own.

It remains to define psychology as a profession and to make clear the difference between the practitioner and the psychotechnologist. A psychological practitioner is a psychologist who earns his livelihood, not by teaching his subject nor by carrying on research, but by helping a client who has specific problems, whether those of an individual, a business firm, or an institution.

Psychology as a science and psychology as a technology have developed *pari passu* within the span of a single human life. When Wilhelm Wundt's laboratory was established in Leipzig, was not Professor Piéron, our own honored leader, already living? Piéron and his colleagues have contributed richly to the development not only of psychology, but also of psychotechnology. The record of growth of both the science and the technology during the past half century can best be traced in the fifty volumes of *L'année psychologique*.

Let me remind you that not only Piéron, but nearly all of the great psychologists with the conspicuous exception of Titchener, have made substantial contributions to psychotechnology as well as to psychological science.

One thinks instantly of what Ebbinghaus did for educational psychology. Pierre Janet was not satisfied to investigate the phenomena of normal and abnormal psychology. Carrying on the torch of Charcot, he lighted the way for mental therapy. Cattell, following Galton, forged the tools with which Thorndike and hundreds of others have studied individual differences in relation to business, industry, education, and vocation. Lightner Witmer, brilliant experimenter, early set the pattern for the psychological clinic. Münsterberg's dreams of a comprehensive psychotechnology applied in

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courtrooms and on the highways as well as in factories, offices, and stores, have to an astonishing degree come true. His facile imagination foresaw the birth of several technologies—among them, personnel management and vocational guidance. Charles S. Myers laid a solid foundation for a psychotechnology of human relations in industry.

As I speak, you are recalling similar examples from other lands and in other areas of application like those of our late Secretary-General, J.-M. Lahy, in the fields of transportation and of industrial management; Edouard Claparède, whose prolific pen left a rich heritage to our technology as well as to our science; Jaederholm here in Scandinavia; Ponzo and Gemelli in Italy; Michotte in Belgium; Lipmann and Stern in Germany; Ishihara in Japan. Not one of these names could be omitted from a history of psychotechnology nor from a history of psychological science. Have not these references to leaders in the two fields already suggested that a psychotechnologist is fundamentally a psychologist?

Without laboring the point, it is further suggested that the *practitioner* of psychology, using the instruments and techniques developed by psychotechnology, likewise must be grounded in psychological science.

The program of papers at the recent International Congress of Psychology in Stockholm beautifully illustrates the wide overlap of these fields. Among 150 titles, I found more than half which would have been appropriate as contributions to a Congress of Psychotechnology. But Professor Katz, the president of the Stockholm Congress, assured me that practically all those papers are contributions to scientific psychology. Very well! We need not dispute the point. Instead, we can willingly grant that we are both right.

Have you not been impressed with the fact that a research designed to solve a specific practical problem has often turned out to be a contribution to science also? And vice versa?

The interdependence between scientific and practical activities within our discipline is neatly illustrated in one of the early inventions of the eminent Swedish-American psychologist, L. L. Thurstone. When an instructor teaching a graduate course in statistics thirty-five years ago, Thurstone chose to illustrate the *j.n.d.* and the equality of equally-often-noticed differences, not by requiring his students to repeat the classical experiments on discrimination of lifted weights, but by letting them

compare things of contemporary importance, as, for example, statements expressive of a person's attitudes toward the prevailing economic system. Thus was produced the first scientifically constructed scale with units known to be of equal magnitude, for use in measuring attitudes such as radicalism-conservatism. This method of scale construction was eventually recognized as valuable alike in general and social psychology, personnel psychology, political science, and business research.

Scales constructed in this way were recently employed, for example, by F. F. Bradshaw, president of Richardson, Bellows, Henry & Co., a New York firm of industrial psychologists. Dr. Bradshaw was engaged by a New Jersey plant to facilitate and improve communication back and forth between the workers, supervisors, foremen, superintendents, top executives, and members of the policy-making board of directors. But before beginning a series of round-table conferences to this end he measured the currently held opinions and attitudes of these groups, and also ascertained what the directors thought the attitudes of their employees were. There was wide disagreement among them. Two or three of the top men held views that were extremely conservative, if not reactionary. A second small cluster was near the opposite end of the attitude scale, while the other members of the directorate were scattered between these extremes. The very conservative directors held the view that most of their employees were very radical.

After completing the program of round-table conferences, alternate forms of the original attitude scales were used, to ascertain whether any significant changes had occurred. The outcome was convincing. There was much more agreement. Among the directors, for instance, none were found near the extreme right or the extreme left of the attitude scale. And their opinions regarding the attitudes of their workers were more realistic. The scales enabled Bradshaw to measure and to demonstrate the amount as well as the direction of these changes.

This interdependence of psychological science, technology, and practice requires that we develop and maintain a common terminology. What the research psychologist discovers, what the psychotechnologist adapts to useful ends, should be described in ways meaningful to the practitioner of the psychological profession, and when possible, to his client also.

Not always has this objective been remembered

by our psychotechnologists. Those who construct new tests and offer them after validation for use in individual appraisal and guidance, have too often grossly neglected their obligation to furnish practitioners with precise information about *the meaning of an individual's score*. Having provided a table of norms and one or more coefficients of validity, they have left the user of the test to estimate very roughly, as best he can, the likelihood that a client who makes a certain score will achieve a designated cut-off point on the criterion scale. This *expectancy*—this number which tells the chances in a thousand or in a hundred that the client will do at least as well as such-and-such members of the criterion group—is not hard to compute, given a coefficient of validity and assurance of linear correlation between the predictor and the criterion. But most practitioners do not have either the time or the statistical self-confidence to compute their own expectancy tables, or to make up their own abacs. These should be computed by the test authors and supplied in the test manuals. Several types of such tables and charts are illustrated in an article called "Expectancies," which I contributed to the Piéron Jubilee Volume of *L'année psychologique*. Such tables are of the utmost service when a practitioner wants to make clear to a person the meaning of his test score, his aptitude index, or any other validated predictor of academic or professional achievement.

To summarize: Psychology as science observes behavior under such conditions of control that our knowledge of behavioral facts and their concomitants is enhanced. The aim of psychology as science is to systematize and to generalize about these facts of behavior. Applied psychology is psychology in the service of aims other than its own. These aims may be personal, social, commercial, political. The content of scientific psychology and of psychotechnology cannot, however, be neatly separated and compartmentalized. The two areas overlap widely.

Psychology as a technology and psychology as a profession are likewise most readily distinguished by their aims. The professional practitioner accepts responsibility for advising a client and helping him to achieve his purposes. The practitioner has at his disposal the resources which psychotechnologists have provided. These include techniques of investigation, of measurement, of diagnosis, of training, and of motivation. A psychologist whose

primary concern is to add to this armament of techniques and to train others to make wise and skilful use of them we call a psychotechnologist or, if you prefer, a technopsychologist. The practitioner is one whose occupation it is to utilize these techniques for the benefit of his clients.

Instances are not rare of psychologists who have won distinction in all three capacities: as scientists, technologists, and practitioners. Many of you in this room from time to time have accepted more than one of these roles.

Of these, the role of psychological scientist is primary. Let us insist always and everywhere, that society must not be compelled to turn for help to practitioners who are *not* first of all psychologists.

And finally, let us do nothing in the field of national or international organization to create or to widen a gap between the scientists and their allies, the practitioners of psychology.

The following paragraphs originally included in the Gothenberg address were omitted from the published version.

This paper ventures to draw the attention of this Congress to a powerful and potentially dangerous trend in the development of psychology, and to offer a single suggestion toward counteracting this tendency.

The danger to which I refer confronts psychologists in every land. We are losing our unity as a profession. We have been caught by powerful centrifugal forces generated by the extraordinarily rapid growth of psychology in our time.

The speed of this development is not always recognized. The science of psychology has achieved maturity within the short space of a man's life—for all of my seniors in this room were living when the Leipzig laboratory was established. We have witnessed not only the growth of an experimental science but also the parallel development of a substantial psychotechnology; and in recent decades the art of professional practice, younger, still without an adequate code of professional ethics and the machinery for its enforcement, nevertheless approaches an ungainly but vigorous adolescence. Psychology is now a profession as well as a science and a technology, a handmaiden of medicine, of education, of social service; and subsequently, of penology, law, business, industry, and government. Most recently, biomechanics—the adaptation of the machine to the worker.

The ramifications of specialization have tended to reinforce powerful centrifugal tendencies.

In the U. S. A. the growth of the *profession* of psychology has been exceptionally rapid, as measured by the number of its practitioners and by the variety of fields in which they are currently practicing. In 1911 I became Secretary-Treasurer of the American Psychological Association. There were found 222 names on the list of members. Several of these were Doctors of Medicine as well as psychologists. A still smaller number were priests or hospital administra-

tors or practitioners of a profession such as architecture who had psychology also as a life interest; but most of us were living academic lives, university professors, earning a livelihood as *teachers of psychology* in institutions of higher learning. I recall only one psychologist who was engaged in private practice with no institutional affiliation. Since then our membership has multiplied 40-fold and the center of gravity has shifted. Of the more than eight thousand members today, less than half are in academic employment. Our salaries as psychologists are no longer earned principally in universities, colleges, or technical institutes, but in social agencies, hospitals, business firms, life insurance agencies, factories, offices, prisons, and government bureaus, or in school systems where the demand is for psychological services, not for class instruction in the subject matter of psychology. In these times, the universities must prepare more psychologists to practice psychology than to teach it.

It is but natural that this rapid growth of psychology,

not only in numbers but in the variety of its responsibilities, should lead to sharp differentiation of interests, to specialization, and to a proliferation of professional societies and journals reflecting these diverse specialties. The danger that impends is the fragmentation of our profession, if these groups choose to go their separate ways, loosening their close relation to the parent stock.

Such a future would be disastrous to each field of specialization which eventually finds itself isolated. Psychology is one. Practitioners, technologists, scientists, all must be psychologists even though some strive primarily to further the aims of the science, while those in fields of application work serve other aims, be they educational, commercial, social, governmental, or the aims of an individual client in search of the fullest realization of his potentialities.

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(This manuscript was submitted posthumously.)

THE MANPOWER SYMPOSIUM AT THE 1952 ANNUAL APA MEETING

IRVING C. WHITTEMORE

Selective Service

ONE of the most impressive and best attended symposia of the 1952 Annual APA Meeting convened Wednesday evening, September 3, in the ballroom of the Mayflower Hotel. Dr. E. Lowell Kelly of the Board of Directors presided. This symposium was scheduled as an Association activity rather than as a divisional project on the ground that the subject matter was of sufficient current importance to justify special scheduling. The panel of speakers included two well-known members of the APA, a physicist-administrator, a research economist, and a soldier-statesman.

From the ranks of the Association came Leonard Carmichael, secretary of the Smithsonian Institution and wartime director of the National Roster of Scientific and Specialized Personnel, and Dael Wolfe, director of the Commission on Human Resources and Advanced Training of the Associated Research Councils. The National Research Council supplied Dr. M. H. Trytten, director of the Office of Scientific Personnel. Dr. Eli Ginzberg, of Columbia University, is director of research for the National Manpower Council. Major General Lewis B. Hershey, a professional soldier for many years and presently the Director of Selective Service, represented Government.

While some of the members of the symposium appeared to be using a script, most did not. In general the atmosphere was informal rather than didactic. Without verbatim stenographic reports it is practically impossible to capture the spirit of a panel discussion. The present account makes no attempt to do so, but rather to reproduce the background that led to the programming of the symposium and the type of thinking stimulated by the contributions which might reasonably constitute its outcome.

In the 176th year of its existence the United States of America faces in many respects the most puzzling international situation which has ever confronted the nation. We are not at war, yet more

American boys have been killed and maimed in the Korean anti-communist campaign than were injured in World War I. For the first time we are faced with the necessity of maintaining a program of peacetime conscription over a period of years. Except in the case of reserve officers and men, who in a sense volunteered for such duty by accepting enrollment in the reserve, the public sense of fair play prevents us from calling upon for military service those who have already put their lives in jeopardy in World War II. For some years the nation's available manpower pool, then, has consisted of the young men who have come of military age since the close of World War II and who have not yet been called upon to serve in the armed forces. The Joint Chiefs of Staff have declared that it is essential for the United States to maintain a total armed force in the neighborhood of 3,500,000. The Congress has declared that no one should serve in the present emergency for more than 24 months against his will, and has also restricted the ages of vulnerability to Selective Service to from 18½ to 26 (unless the individual has been deferred for some reason, in which case his liability is extended to age 35).

As a consequence, the demand of the armed forces for personnel since the outbreak of aggression in Korea has resulted in the gradual diminution in the available manpower resources of the nation until it is expected that in the not-distant future our sole reliance will have to be upon the 18½-year-olds in each successive year.

Only the most prejudiced are likely to disagree with the statement that America's greatest contribution to victory over Germany and Japan in 1945 was its contribution of material and supplies. To a greater extent than was true in almost any other nation, with the possible exception of Great Britain, American manpower was deferred from service in order that the productive capacity of the country should not be diminished by a failure of numbers. German prisoners of war never conceded the su-

periority of our military methods but they were aghast at the prospect of having to continue a struggle against the types of material which they saw on the roads of Europe in the hands of American soldiers and their allies. In a very real sense it was the factories at home which won the war rather than the soldiers on the firing line.

In a temporary emergency, such as the major wars fought by the United States have constituted, there has not yet been a compelling need to replenish the scientific and technical brainpower responsible for the productive effort. Although England made some effort to direct the best technical minds into channels in which these minds might be of greatest use to the nation, the United States did not, during the period 1940 to 1945, feel it necessary to resort to a similar major diversion of young intellectuals from military service. In the present situation, which is neither war nor peace, those responsible for the mobilization of the country have had to plan for the maintenance of our current intellectual and technical as well as military resources. No one can doubt that a good atomic scientist in 1944 was worth a division.

It was the problems intrinsic in the situation represented by this review to which the members of the American Psychological Association Symposium on Manpower addressed themselves.

DR. CARMICHAEL

Dr. Carmichael opened the proceedings with the observation that if there were a scarcity of laboratory chimpanzees it would be possible to assign them to various research institutes and to make the best possible use of them there, but when it comes to human beings with their feelings and hopes, the chimpanzee solution is not satisfactory. Any program must be flexible and we should maintain a current census of the intellectual resources of the nation. He believed that the Department of Defense should be made to state its manpower requirements in realistic terms, that personnel should be "screened in" not out. There are numerous situations in which 20/20 vision is not necessary for the performance of important military duties. We need manning tables, but it is admittedly difficult to project our needs into the future and these manning tables should be constantly revised. He declared that the Selective Service College Qualification Test procedures represent a desirable method of screening the persons most qualified to carry on the de-

velopmental and research programs necessary to the security of the nation.

There is no simple formula in accordance with which "equality of sacrifice" may be attained, nor is this equality of sacrifice necessarily desirable. Some people would be more effective in a military role after rather than before training. In other cases the reverse would be true. The essential question to be asked is where may the individual most appropriately make use of such skills as he possesses.

DR. WOLFLE

Dr. Wolfle reviewed the statistics concerning the quantity and nature of the special skills upon which reliance must depend for research and developmental activities. He pointed out that in our present culture college training is necessary for most "specialties." About one in eight young men and women of college age actually graduate from the colleges—perhaps one in five start.

Where the average level of performance on the Armed Forces Qualification Test is represented by a score of 100, average young persons graduating from a higher institution of learning score 121. This score is probably slightly higher in the case of university and liberal arts graduates, say 126. It is true that some 10 per cent of the people who attend college score 100 or below on the AGCT. On the other hand, only 37 per cent of the total population scoring above 120 actually complete college. Nearly two out of three in this group do not graduate because of lack of money, or of preliminary training, or of interest. Of the top 2 per cent in AGCT scores about two-thirds finish college, of the top decile nearly half, and of the top 33 per cent, about one-fourth finish college.

Many young men with AGCT scores of over 120 go into the armed services voluntarily, some through the ROTC, so that it cannot be said that present Selective Service policies deprive the armed forces of all the bright young men.

There can be no question of the increasing demand for specialized personnel. In 1900 only 4 per cent of the total population was engaged in activities which might be so classified. In 1950 the proportion had risen to 9 per cent.

In the long run our best preparedness will rest upon adequate numbers of trained personnel available to the civilian economy and to the armed forces.

DR. GINZBERG

Dr. Ginzberg opened his remarks with the observation that, since economists have not yet solved the problem of materials, we can scarcely expect them to solve in addition the problem of manpower. He confessed himself somewhat puzzled by the meaning of the term *manpower utilization*. Take, for example, the problem of training. Training for what? A lot of engineers become executives. Does utilization imply a time element? Do we mean that a person who has not worked the maximum number of hours per week of which he is capable is malutilized? When we speak of the proper utilization of manpower are we referring to the utilization of the powers of the man himself as distinct from other persons, or as a member of an organization? There arises also the question of values. When we promote officers in the medical corps to the grade of *general*, are we utilizing their professional competence or are we converting them into something other than physicians? Many young men in all kinds of work have to wait for promotion when qualified by both experience and capacity to work at a level above that which their assignment calls for. Are these young men being improperly utilized or not?

In relation to the problem of utilization there are five moral dilemmas: (a) Are the controls characteristic of professional training essential except to protect levels of pay? (b) When associations of specialists require conformity to a fairly rigid ethical and professional code, does the degree of conformity demanded mask prejudice or is it actually in the best interests of the profession and society? (c) In relation to the problem of utilization there will always be conflicts as between age and youth, the problem of seniority. (d) Is the leader always superior to the follower? (e) What is the criterion of utilization with respect to women? Is the "double standard" with respect to pay and responsibility justifiable? Utilization always implies norms, and these mean of course that there are social and moral issues involved. In this country manpower as a problem is relatively new. We have only recently become concerned with making the best of each man.

DR. TRYTTEN

Dr. Trytten traced the origin of the setting in which present manpower problems must be considered. According to him it is unusual in the his-

tory of America—compulsory service in time of peace is relatively new. Our present experience is based largely upon that of World War II, but the goals of the present mobilization are different.

We must maintain armed-forces-in-being of a certain minimum size, and in addition provide for the training of increments thereto. The over-all goal is the development of maximum national strength, not the creation of armed forces for their own sake. The authority to mobilize is contained in the Universal Military Training and Service Act of 1951. This legislation sets the age and time in accordance with which compulsory service may be required. It establishes ground rules. For the maintenance of an armed force of around 3,500,000 men, some one million will be required annually as replacements if the maximum period of service for those inducted is 24 months. About 1,000,000 men come of age each year. Of these only some 750,000 to 800,000 are likely to be acceptable to the armed forces in terms of physical and mental health. So far, the pool of men built up as a result of the conditions obtaining between 1946 and 1950 has been sufficient to take care of annual replacements. This reserve has dropped from some 3,500,000 to 1,200,000 since Korea. In all probability it will disappear within a year to eighteen months. When the pressures for manpower become stronger than they are at the moment, the whole concept of student and occupational deferment will be at stake. The central problem confronting us at the moment is how we can preserve the principle of such deferment in the public interest in the face of the need for military manpower.

GENERAL HERSHEY

General Hershey was the final speaker on the program. To anyone who has heard him in one of his public appearances it will be easy to understand why a report of his remarks often fails to convey their meaning. The General is highly adaptive to the reactions of his audience and intersperses his statements with witticisms and homely illustrations that seem discursive but usually end up in exactly the impression he has wished to achieve. His style makes his observations difficult to paraphrase, and to condense them is likely to lead the reader to a conclusion quite different from the listener's.

On this occasion the General spoke for some time of the over-all problem in terms of manpower po-

tential, the gradual reduction of that potential, and the practical consequences resulting therefrom in terms of the availability of "bodies" and the nature of anticipated public reaction.

He approved discussion of the problem because it has become his conviction over the years that in a democracy sound policy can result only when the people know and react to all major issues, and nothing is closer to the heart of the people than the use of its sons in war.

He said he sometimes fears that the boys who are able to avoid military service are the ones who know either too little or *too much*. The percentage of persons rejected as mentally incompetent by the Armed Forces Examining Stations is alarming. The Selective Service System has no control whatever over this rate of rejection, although few people seem to realize it. He pleaded for a more realistic appraisal of both mental "competence" on the one hand, and military serviceability on the other. It was clearly his conviction that psychologists have a responsibility not merely to determine scores on a mental test, but to interpret and evaluate these scores in such fashion as to disqualify fewer persons for military service for mental reasons than is now the case. In substance he chal-

lenged the profession with the words "Let's get some of the *flatheads!*"

Following the speakers some members of the audience raised questions from the floor, but none which substantially altered the scope and tone of the discussion.

SUMMARY

While the outcome of a symposium can never be stated in the exact terms of a piece of carefully designed research, it is reasonable to suppose that listeners carried away from this meeting some fairly concrete impressions. For example, there was general agreement among the participants that (1) a manpower problem existed, (2) the relation of scientific and other skills to this problem is an important aspect thereof, (3) a solution in a democracy must rest on a subtle pattern of values, and (4) the problem is due to become more rather than less difficult in the near future. An objective solution to the military and civilian manpower problems of today is obtainable if those with competence in the field think constructively and speak convincingly to the men of action.

Manuscript received October 10, 1952

Comment

Psychologists in Hawaii

The October, 1952 issue of the *American Psychologist* contains an article by Robert P. Clower entitled "A Preliminary Survey of the Distribution of Psychologists in the United States." Data for the 48 states and the District of Columbia are tabulated and discussed, but Hawaii is not mentioned. Since we in Hawaii are very much a part of the United States, and especially since there is good reason to believe we shall constitute the 49th state in the near future, I submit the following data to supplement Clower's article.

The population of the Territory of Hawaii in 1950 was 471,447. The 1950 APA Directory listed 26 psychologists. The number of psychologists per 100,000 population was 5.5, a figure which considerably exceeds that for the continental United States and exceeds that for 40 of the 48 states.

Eighteen of the 26 psychologists listed hold the PhD degree, 7 the MA, and 1 an MD. The percentage holding the PhD is 69.23, which is higher than that for 42 states.

Seventeen of the 26 psychologists listed were affiliated occupationally with the University of Hawaii, one was employed by the Federal Government, and three were in private practice. The percentage engaged in private practice, 11.54, exceeds that for any of the states reported in Clower's article and is 4 times higher than that reported for the continental United States as a whole.

HERBERT B. WEAVER
University of Hawaii

Scientific Writing

My congratulations and personal thanks for the article by Hebb and Bindra, "Scientific Writing and the General Problem of Communication" (*Amer. Psychologist*, 1952, 7, 569-573). This discussion is the first within my ken to bridge the gap between the artist who shuns analysis and the analyst who does not recognize artistry.

As one who is neither a polished artist nor an accomplished scientist, but who aspires to be a bit of both, I should like to toss in a few supplementary remarks. Fowler's *Modern English Usage* should indeed be the writer's Bible. Those intent upon improving their styles could also read, with profit, Emerson's essays and Conrad's novels. Despite long sentences and relatively little human interest content, Emerson is quite readable because of the majestic rhythm of his prose. Conrad's short sentences and simple constructions achieve an equally pleasing rhythm. Read close

together, Emerson and Conrad offer an interesting contrast. Perhaps the greatest technician the English language has ever known is Logan P. Smith. His three books of essays, *Trivia*, *More Trivia*, and *Still More Trivia*, are examples of triple-distilled style. Their content is, as their titles imply, merely a vehicle for elegant arrangements of words.

Those scientists who lack the time, energy, or interest to polish their use of English would be well advised to adopt a simple formula: declarative sentences without subordinate clauses. And above all they should eschew the passive voice, which leads to clumsy constructions. Thus stripped down, English may be monotonous, but at least the reader can progress swiftly, cheered by the hope that the next article may be more engagingly composed.

GEORGE A. W. BOEHM
Science Editor, *Newsweek*

Audiovisual Aids

The increased use of audiovisual aids poses a question to Robert Tyson (*Amer. Psychologist*, 1952, 7, 546) of how best to meet this threat so as to prevent the teacher from becoming either an "auxiliary" or from being "displaced." I wonder if this threat is more imagined than real.

Perhaps our Mental Health Society's experience with audiovisual aids on a community level may enlarge the scope of the perceived relationship of the teacher to these media. In general, I feel that, rather than narrowing or delimiting the role of the "me" in the me-other relationship, more and more requests are made for the personnel of our clinic as well as the Mental Health Society because of our initial reliance on audiovisual aids.

In addition to discussion groups, panel discussions, buzz sessions, seminars, and institutes, we are constantly using films, radio programs, tape recordings, psychodrama and role-playing techniques. Our educational program has given us an entree to the local citizens' homes, the high schools, the local college, PTA groups, teacher institutes, mother study groups, personnel clubs, the Chamber of Commerce, the churches. In fact, our community relationships involve almost every local institution and organization.

But more important has been the challenge of the continuing growth of our educational program. Initially, it was primarily the clinic staff member who conducted a program, built around a film, and sometimes a skit. With increased demands on clinic time, our need for more leaders is being met by using lay

people, teachers, ministers, mothers, board members, and volunteers. This, in turn, has led to an awareness of the need for leadership training programs which stress the meaning of mental health, program planning, and group dynamics.

At present, we feel there is a need for still more intensive training. We hope to use one of the local schools as a demonstration project for a "total push" mental health program. Then, too, we hope that the time is not far distant for more intimate work with teachers regarding the importance of their role in the classroom. Such a program may well take on a group therapy relationship. Finally, we plan intensive mother study group programs that will extend over a period of several months with one group.

Instead of limiting our role, the use of these various educational methods has extended our previously conceived vistas of mental health education. They have focused the need for more intensive rather than extensive educational programs. However, I do not feel that the value of these media is intrinsic. Rather, to a large extent, the responsibility for their use rests with us as individuals. I feel strongly that instead of becoming an "auxiliary" to this mechanization or feeling any impending doom or disaster, the use of these media has led, rather, to the use of other auxiliaries (personnel and programs) to keep up with the demands generated by group awareness.

HARRY CONSTANTINE

Mental Health Clinic, Rockford, Illinois

What Is the "Real" Problem?

During the depression of the nineteen-thirties, a counseling psychologist of Freudian orientation was beginning her fourteenth interview with an unemployed male client. With rapport properly established by the proffer of a cigarette and a few pleasantries about the weather and the ball game, she put on her professional smile and inquired, "What shall we talk about today?" Her client responded, "Well, I think we ought to talk about how I'm going to pay my rent, but if you want to talk sex it's OK with me."

Today our preoccupation with sex is a little less complete than it was a few decades ago, but many of us still seem to base our counseling on a series of unverified assumptions which run something like this:

1. Every client has both a "surface" problem and a "real" problem.
2. Every client first presents his "surface" problem.
3. The counselor, not deceived, proceeds patiently to help the client to discover, recognize, reveal, and face his "real" problem.
4. When the "real" problem has been identified the counselor helps the client to learn how to deal with it effectively.

5. With the "real" problem thus disposed of, the client is able to solve his "surface" problem without the counselor's aid.

There is enough truth in these assumptions to lend them a spurious plausibility. There is enough evidence to indicate that the assumptions are valid for some clients. What leads the detached observer to raise an eyebrow is the frequency with which the "real" problem proves to be the problem which most interests the counselor and with which he feels most competent to deal.

This human tendency to project ourselves was neatly demonstrated¹ as far back as 1914, when twelve skilled and experienced male social workers interviewed some 2,000 consecutive homeless applicants for admission to the New York Municipal Lodging House. Each applicant was separately interviewed for twenty to thirty minutes, during which time the interviewer sought to determine the real cause of the applicant's destitution. One of the twelve interviewers was an ardent believer in prohibition. He found "liquor" to be a major or minor cause of destitution in 78 per cent of the cases that he interviewed; he found industrial circumstances, such as "lay-off," "seasonal work," or "shutdown" to be a major or minor cause in 29 per cent of the cases. Another interviewer was regarded by his associates as a socialist. His corresponding percentages for the cases he interviewed were 37 for liquor and 73 for industrial circumstances.

Would we not be more scientific in our approach to counseling if we regarded with more suspicion our tendency to identify the real problem as the one which interests us?

ROBERT HOPPOCK

New York University

Things of Science

Science Service puts out a monthly demonstration or experimental kit that is of interest to psychologists in two ways. As a well-designed series of simple experiments on diverse aspects of pure and applied science, it represents a significant attempt at extracurricular science education for children (and their parents). But it is equally interesting as an inexpensive source of material and equipment for large elementary courses in psychology. Several recent kits come under this classification:

No. 98 (Dec., 1948). Color Unit. Contains a substantial color top, with graduated disc, seven (Munsell) colored discs, and a red filter. The booklet, prepared with the aid of Dr. Deane B. Judd, gives adequate instructions for 28 simple experiments, including color

¹ RICE, STUART A. Contagious bias in the interview. *Amer. J. Sociol.*, 1929, 35, 420-423.

mixing, contrast, effects of illumination, and even brightness constancy.

No. 130 (Aug., 1951). Color Vision Unit. Contains a Pseudo-Isochromatic Plate for testing color perception (American Optical Co.), material for a color confusion chart (designed by Lt. Comdr. Dean Farnsworth), and a metameric pair (two samples of cloth, both green in daylight, one of which becomes brown in Mazda light). The booklet gives 14 experiments, which are somewhat less traditional than those in No. 98.

No. 140 (June, 1952). Highway Safety Unit. Contains a Stimsonite directional reflector, as well as reflective fabric, sheeting, and beads that can be used with paint. The 18 experiments include binocular depth perception, field of vision, dark adaptation (including monocular), and reaction time. Most of the experiments are due to Dr. T. W. Forbes, but the present writer contributed a description of how to convert a meter stick into a silent and accurate chronoscope (after Piéron).

No. 144 (October, 1952). Visual Illusions Unit. In addition to many of the standard geometrical illusions, it contains several of the Ames demonstrations: trapezoidal window, a plastic aniseikonic lens, and a cut-and-paste pattern for a distorted room. The 23 experiments were prepared with the aid of the Institute for Associated Research, Hanover, New Hampshire.

No. 145 (November, 1952). Taste Unit. It contains samples of citric acid, sugar, rock salt crystals, quinine, magnesium sulfate, P.T.C., and chewing gum. The 16 experiments cover taste, taste blindness, and the distinction between taste and smell. The Arthur D. Little Co. helped with the selection of specimens, and Dr. Albert F. Blakeslee invited subscribers to report data on taste blindness to him.

Although subscriptions are handled on an annual basis (\$5.00), kits may be purchased singly at \$.75 each, or three for \$1.50. In general only enough of these kits are issued to supply members, but there is usually a small overrun. Only a few of No. 98 and No. 130 are available at this writing, but a sufficient quantity of No. 140, No. 144, and No. 145 are on hand to meet even multiple demands. Order Things of Science from Science Service, 1719 N Street N.W., Washington 6, D. C.

HAROLD SCHLOSBERG
Brown University

Classification of Papers at the 1952 APA Meetings

I have been amusing myself by attempting some classification of the abstracts of the papers presented at the Washington APA meeting. It seems obvious that any purist who has preconceptions of what constitutes a proper subject matter for psychology will be shocked at the seeming irrelevance of the majority of

the papers! Judging from these papers there is not one science called psychology but instead several sciences (social, biological, applied, and otherwise) all of which are called psychology by someone.

Even though sheer quantity of work is no indication of the ultimate importance of any topic, the following breakdown of these abstracts indicates that the applied areas are now of great interest to psychologists.

	No. of Abstracts
Applied	
Educational	29
Clinical and counseling	70
Industrial and public service	17
Military	33
Geriatrics	20
Total	169
Semitheoretical	
Human learning and performance	25
Child development	10
Social	49
Personality	13
Measurement and testing	54
Total	151
Theoretical	
Animal learning and motivation	38
Brain function	4
Perception and sensation	22
Esthetics	4
Total	68
Unclassified	3

Since symposia and discussions are primarily theoretical, perhaps this classification, which ignores them, does not do justice to the theoretically-minded psychologists who attended the 1952 meeting.

HAROLD WEBSTER
University of Kentucky

The Ages of Scheduled Participants at the 1948 APA Annual Meeting

According to the July, 1948 issue of the *American Psychologist* 458 individuals were scheduled to participate in the 1948 annual meeting of the APA. Since the 1948 *Directory* listed a total of 5,047 members, it is apparent that less than 10% of the total membership participated formally at the Boston meeting.

One of my students, Mr. Bernard A. White, assembled statistics with reference to that participation which permitted the following analysis. He divided the participants into the two following groups: (a) those who read papers, and (b) those who participated in ways other than the reading of papers, i.e., participation in symposia, addresses by the APA president and by the divisional presidents, the showing of slides and films, and miscellaneous "other" formal participation. The

next step was to ascertain the ages of all participants and also the number of APA members by age-group. The ages of 6% of the participants and 11% of the total APA membership could not be found readily. The following findings are based therefore upon 94% of the participants and 89% of the total membership.

In constructing the graphs that appear in this article, the data for each of them were first reduced to a comparable basis by the following procedure. The peak of each statistical distribution was arbitrarily assigned a value of 100%, and the other averages within the same statistical distribution were assigned proportionate percentage values. For example, in Fig. 1 the peak of the distribution occurred at ages 30-34, inclusive. At this age interval the average number of papers read per APA member was .0902. In Fig. 1 the decimal .0902 is plotted therefore as 100%. At the age interval 60-64, inclusive, the average number of papers read per APA member was .0231. This latter decimal fraction is equivalent to 26% of the maximum (26% of .0902) and in Fig. 1 the decimal .0231 is therefore plotted as 26%.

For the entire APA membership the average number of papers read was .0539 per member. This latter decimal fraction is equivalent to 60% of the maximum (60% of .0902). In Fig. 1 the horizontal broken line is drawn at the 60% level. If each age-group had read papers in proportion to its numerical strength, the solid line of Fig. 1 would coincide with the horizontal broken line.

But the successive age-groups did not read papers at the Boston meeting in proportion to their numerical strength. The solid line of Fig. 1 reveals on the contrary that papers were read *more* often than would

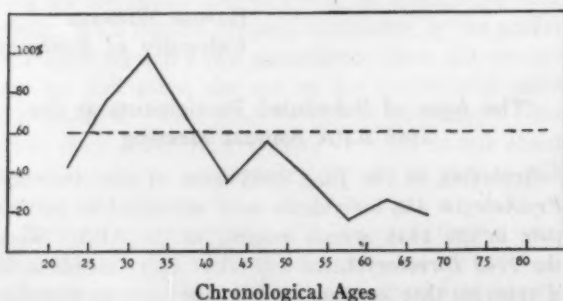


FIG. 1. Solid line: Number of papers read by members of each age-group, in proportion to membership, at the 1948 meeting of the APA. Broken line: How the solid line of this figure would appear if each age-group had contributed papers in proportion to its numerical strength.

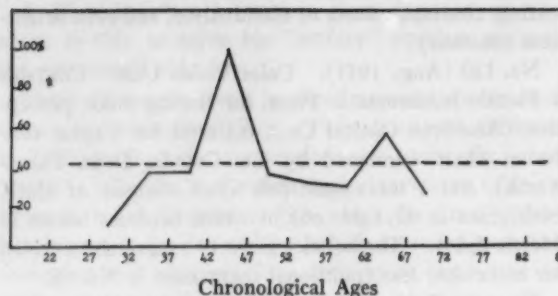


FIG. 2. Solid line: Number of program participations, other than the reading of papers, by members of each age-group, in proportion to membership, at the 1948 meeting of the APA. Broken line: How the solid line of this figure would appear if each age-group had participated in proportion to its numerical strength.

have been expected on the basis of their numerical strength by age-groups 25-29, 30-34, and 35-39. Papers were read *less* often than would have been anticipated by each of the remaining age-groups.

In Fig. 2 the solid line shows by age-group the number of participations other than the reading of papers. If these other participations had occurred in proportion to each age-group's numerical strength, the solid line of Fig. 2 would coincide with the horizontal broken line drawn parallel to the base line of Fig. 2. Figure 2 reveals that these "other" participations occurred *more* frequently than would logically have been anticipated at two age levels only, namely, at ages 42-46 and at ages 57-61.

It has often been remarked that the young men read the papers at the annual APA meetings and that the older men do not. In so far as the present writer is aware, data with reference to this problem have not previously been assembled. Apparently, prestige enables older men to participate generously in ways other than the formal presentation of research findings. If it may be assumed that the reading of papers is a reasonably fair measure of voluntary creative effort, Fig. 1 suggests that most of the age differences in psychology (and possibly in other fields as well) may be due largely, if not solely, to age differences in motivation and effort. Unfortunately, the problem of age differences in creativity is not as simple, nor can it be solved as easily, as the foregoing assumption might lead one to suspect.

HARVEY C. LEHMAN
Ohio University

Across the Secretary's Desk

Psychological Resources in the South

On the Emory University campus and under the auspices of the Southern Regional Education Board, approximately 110 psychologists, psychiatrists, and social workers conferred for three days late in January on the general problem of the South's need for psychological resources and its ability to meet that need. The conference opened with addresses by the Honorable Frank Clement, the 32-year-old governor of Tennessee, and by Dael Wolfe, of the Commission on Human Resources. Then group dynamics set in. Small groups organized themselves first to set and then to wrestle with the agenda for the conference. On the final day, all the small groups became again a large group to take action as a conference and to hear the APA Executive Secretary "summarize." Discussion on the recommendations from small groups became so lively and action so intricate that the "summary" became, happily, a two-minute affair consisting of one small pleasantry and two pious generalizations.

There will be a formal report from the conference, available in some form to interested psychologists. The problems confronted were very similar to those facing psychologists everywhere, e.g., the various facets of training, relations with other professions, participation in community affairs, research facilities, legislation, use of the psychologist's title, and reciprocal relations between the university and the practitioner. The Southern attack on these problems can in some ways be unique. For one thing, there appears to be less interprofessional rivalry in the South than in other parts of the country. For another, there can be, through the experienced offices of the Southern Regional Education Board, cooperative efforts and pooling of resources on the part of a number of universities in various Southern states.

It appeared to many participants that the conference was a productive one. It is a good hypothesis that the objective observer would conclude that Southern psychologists are good psychologists and that psychology in the South is developing healthily under their guidance.

A Committee Looks at the Central Office

From 9 A.M. to 7 P.M. on Saturday, February 7, a special APA committee sat in the Board Room of

the headquarters building and studied the operations of the Central Office. They talked with our staff, asked questions, took notes, had ideas, ate a free lunch, and wrote a report to President Shaffer.

We here do not know the content of the report or whether it will be published, but we can react to the idea behind the appointment of such a committee and to the experience of being studied by one.

At the September 1952 meeting of the Board of Directors some specific questions concerning the Central Office were briefly considered and referred to the President for action. During the fall, the President and the Executive Secretary talked and corresponded about these things. It became clear that the specific problems could not really be solved until more is known about the general purpose and functioning of the Central Office.

The idea of a Committee of Assessment gradually evolved. It appeared desirable to have a few APA members, not at present intimately involved in our government, come in and take a close look at what the Central Office does and how it does it. The President appointed Donald Baier, T. G. Andrews, and Denzel Smith to the committee. He asked Donald Baier to serve as chairman and Carroll L. Shartle to sit in as an *ex-officio* member.

All morning the committee interviewed the key people in the office, the people we have recently been referring to as "department heads." There were many questions about ordering supplies, keeping records, editing journals, handling mail, maintaining the building, addressing journals, and editing the *American Psychologist*; about addressograph machines, directories, public relations, membership applications, employment bulletins, flow of visitors, and back issue sales; about relations with the Board of Directors, service to divisions, conduct of business with printers, work for APA committees, contacts with governmental agencies, travel, salary scales for clerks, internal communications, and personnel policies.

During lunch and the first part of the afternoon, the committee studied the activities of the Executive Secretary. They found out how much time he spends on what sort of correspondence, about his working relations with other people in the office, about his staff work for boards and committees,

and about his relations with the "outside world." Both the committee and the Executive Secretary had considerable difficulty in fitting a description to the Secretary's job—as it is or as it should be. But both the committee and the Executive Secretary developed pictures of various possible "executive patterns" and began to think constructively about what sort of pattern might now be best for advancing the purposes of the APA.

Those of us who work here have long had the general conviction and ambition that APA should have—deserves to have—the most effective administrative office in the world. We ought to get a great deal of work done with a great deal of efficiency. We ought to have an office in which one can enjoy working. Our interpersonal relations ought to be on a mature level. We ought to find and maintain a proper balance between structure and flexibility, between informality and organization, between permissiveness and directiveness. We ought to maintain a warm and helpful attitude toward our members and we ought to have a great sensitivity to feedback from a wide variety of psychologists. We ought to be able to do what we conceive to be our jobs in the face of occasional criticism and hostility. And those of us who work at the professional level should come into the office for a relatively few years, do our jobs with all our might and according to our own best lights and then get out, making way for other "amateurs" with other ideas, other personalities, and other professional identifications.

We here do not cultivate the illusion that we have an ideal office. We do think it is a good office but we are sure it can be improved. We think the visit by the committee will result in improvement.

Though no drastic changes in attitude or function have yet occurred, several of us have been doing a new sort of thinking and feeling about our jobs. We feel as if we have gained perspective. It is a very absorbing thing to work for APA. The problems that come at us through the mail and over the phone or in the hands of visiting members are interesting, intricate, demanding, and almost infinitely varied. Normally we sit down at our desks in the morning, bend our heads to today's tasks—tasks that are formulated and scheduled by some-

one outside the office—and come up for air only at nightfall. The visit of the committee meant that our heads were up for a full day. We began to see our jobs through outsiders' eyes. And we are quite convinced that even brief conversations with concrete psychologists create more perspective than protracted communion with a generalized other. Aside from the several specific ideas we picked up from the committee, this increased perspective should result in an improvement of our general Central Office function, should increase the quality of our service to APA members, and decrease the likelihood that we here will, while neither we nor others are looking, build up a routinized and non-functional bureaucracy.

Another general effect of the committee's visit was that we here feel much less lonesome. There are not very many APA members who really know what we do nor many who are very interested. The committee's presence represented for us a regular orgy of shop talk. At least three APA members, not before connected with or exposed to the Central Office, now know what we do and what we think. It feels good. It feels good even if we suspect the committee does not approve of all we do and think.

It seems a very healthy thing that APA members should periodically audit their Central Office. We here are hoping the Association will establish it as a policy that a group of members annually takes a careful look at what does and does not go on in the office. Such an annual assessment will not only bring bright ideas to any current Central Office staff but will represent good insurance that the office will stay functionally resonant to the real interests of those who support it.

Spring Meeting of Board of Directors

The Board of Directors will meet on April 9, 10, 11, and 12 in the headquarters building in Washington. Individual members, committee chairmen, members of Council, or divisional officers who wish to insert items into the Board agenda should communicate before April 1 either to a member of the Board or to the Central Office.

FILLMORE H. SANFORD

Psychological Notes and News

Richard M. Magoun of Florida Southern College died September 3, 1952 at the age of 30.

Kate Franck died in Sydney, Australia in December, 1952. She was 46 years old.

William L. Jenkins was killed in an automobile accident on January 1, 1953 at the age of 34. Dr. Jenkins had just been designated chief clinical psychologist after having served on the staff of the VA Hospital in Minneapolis, Minnesota.

David Katz of Stockholm died of a heart attack on February 2, 1953. He was born October 1, 1884.

At the Catholic University of America James Van der Veldt has been appointed full professor and Katherine Keneally, associate professor. Newly appointed lecturers are Otmar Solnitsky of Georgetown University, Leon Salzman of the Washington School of Psychiatry, Robert Stubblefield of the USPHS, Reginald Lourie of Children's Hospital, and Edmund Fuchs of the Personnel Research Section, AGO.

John W. Thibaut, who has been lecturer and research associate in the department of social relations, Harvard University, has accepted an appointment as associate professor of psychology and associate professor of sociology and anthropology at the University of North Carolina, effective March 1953.

Frank A. Beach has been elected president of Section I of the American Association for the Advancement of Science.

John Paul Nafe has been appointed Distinguished Professor of Psychology at Florida State University for the second semester. He will teach a course in perception and engage in research at the animal laboratory with Howard Baker and Daniel Kenshalo.

Mildred E. Leonard has accepted a position as testing supervisor in the employment department of Macy's Herald Square, New York City. She was previously the administrative assistant in the Psychological Services Center of Syracuse University.

The department of psychology of the Springfield State Hospital in Maryland now consists of the following staff: Michael H. P. Finn, chief psychologist; Donald S. Milman, William H. Guy, and Julian Abrams, staff psychologists; and Saverio Salvati, Bert Friedman, Robert White, and Paul Imre, psychologist interns.

Murray Wexler, who has been senior staff psychologist at Connecticut State Hospital, has joined the faculty of the department of psychiatry of the State of New York School of Medicine in Syracuse.

Grace B. Cox has been appointed chief of psychological services in the Bureau of Mental Health of Pennsylvania's Department of Welfare.

Alfred M. Comens was recently appointed to serve as the Director of Civilian Personnel for the Department of the Army servicing the civilian employees in the Military Districts for the five state area of Minnesota, Iowa, Nebraska, North Dakota, and South Dakota. His offices are in Headquarters Minnesota Military District, Minneapolis, Minnesota.

William S. Verplanck will spend the spring and summer on sabbatical leave from the department of psychology, Harvard University, working in the Zoological Laboratories, Oxford University, with N. Tinbergen. His work is being undertaken with the support of a grant by the American Philosophical Society.

Monroe Stein has been appointed chief psychologist at Western State Hospital, Fort Supply, Oklahoma, effective December 24, 1952.

At Milledgeville State Hospital, Milledgeville, Georgia, John T. Rowell has been appointed chief clinical psychologist. Henry Raymaker, Jr. has joined the staff as junior clinical psychologist, and Mary Fretwell and Henry B. Adams as senior psychologists.

William McGehee, industrial psychologist at the Fieldcrest Mills, Leaksville-Spray, N. C., has been appointed visiting professor in industrial psychology in the department of psychology at North Carolina State College. This is to be a continuous

appointment, and Dr. McGehee will teach an advanced class in industrial psychology each quarter.

Gordon L. Lippitt, assistant director of the National Training Laboratory in Group Development at the National Education Association, has been granted a one-year leave of absence to serve as Education and Training Specialist for the Mutual Security Productivity and Technical Assistance Program in Europe. He will be stationed in Paris. During his absence Jack Gibb of the University of Colorado will serve part time in the Washington Office of the National Training Laboratory in Group Development.

Newman L. Hoopingarner, professor of business psychology at New York University, has been elected chairman of the Psychology Section of the New York Academy of Sciences for the 1953 term.

Harold H. Anderson, head of the department of psychology of Michigan State College, has been granted a six-months' sabbatical leave, beginning January 1, 1953. He and his wife, Gladys L. Anderson, will spend most of the time in Mexico City continuing their "Intercultural and Developmental Study of Children's Social Perceptions." They will also complete a report of research with adolescent children begun in Germany last summer. Comparisons will be made with data obtained from children in Germany, France, Mexico, and the United States. During his absence Donald M. Johnson will serve as acting head of the department. New appointments in the department of psychology at Michigan State College effective September 1952 are: Harry A. Grace and G. Marian Kinget, assistant professors, and James J. Gallagher and Susanne M. Poch, instructors.

Arthur H. Davison has resigned from the staff at the Veterans Administration Hospital, Perry Point, Maryland and has been appointed chief psychologist at the Milwaukee County Hospital for Mental Diseases, Milwaukee, Wisconsin.

DeWitt E. Sell, formerly director of psychological services, Ohio State Reformatory, has been appointed to the newly created position of supervising psychologist, Division of Corrections, Ohio Department of Public Welfare. He will have supervision over all psychological services in Ohio's adult penal institutions. Sheldon B. Peizer, who has been on the staff of the Ohio State Reformatory,

has been appointed acting director of psychological services for the institution. S. Lee Whiteman has been added as a staff psychologist at Ohio State Reformatory. He recently completed his master's work at Kent State University.

Seymour W. Beardsley completed a temporary assignment for the State Department, in Germany, last June, and is now connected with the Washington Seminar of the American Friends Service Committee, as research associate. The office is located at 1333 Sixteenth Street N.W., Washington 6, D. C.

Robert Guion has recently joined the psychology staff at Bowling Green State University. Dr. Guion will be in charge of developing a program leading to the degree of master of arts in industrial psychology. Recent improvements in the physical facilities include a fully equipped projection room, photo laboratory, and the addition of several individual research rooms. A cooperative arrangement has been completed with the Ohio State University whereby up to one year of work beyond the master's level at Bowling Green State University may apply to the PhD program at the Ohio State University, contingent upon the acceptability of the candidate to both institutions.

ABEPP Announcements

The American Board of Examiners in Professional Psychology, Inc. has conducted partial and/or complete oral examinations at the following centers during the calendar year 1952: Los Angeles and Berkeley, California, April 28-29; Minneapolis, Minnesota, May 1-2; New York City, July 15-16; Topeka, Kansas, July 25-26; New York City, August 8; Washington, D. C., September 2-4; Chicago, Illinois, October 16-17; Berkeley, California, November 8-10; New York City, December 9-10.

The total oral examination included a Professional Field Situation and the following four parts:

I. Client Relations:

a. Diagnosis or evaluation. (The definition of the psychologist's professional problem.)

b. Therapy and/or recommendations. (The solution of the psychologist's professional problem.)

II. Scientific and Professional Relations:

c. Skill in the interpretation and use of research findings. (What valid knowledge exists regarding professional practice.)

d. Organization and administrative problems of professional psychology. (What are the conditions of professional practice.)

The Board wishes to express its appreciation to the following diplomates who served as members of its oral examining teams for the above mentioned examinations:

In clinical psychology, Carl L. Anderson, Benjamin Balinsky, Nancy Bayley, Samuel J. Beck, Joseph M. Bobbitt, Hedda Bolgar, Katherine P. Bradway, Olga Bridgman, Rudolf Ekstein, Erika A. Fromm, Dorothy S. Fuller, William M. Hales, Florence C. Halpern, Eugenia Hanfmann, Albert J. Harris, Robert E. Harris, Molly Harrower, Starke R. Hathaway, Ruth M. Hubbard, Howard Hunt, William A. Hunt, E. Lowell Kelly, Isabelle V. Kendig, Seymour G. Klebanoff, Helen L. Koch, Mary S. Kunst, Samuel B. Kutash, Bernard Locke, Jean W. Macfarlane, Solomon Machover, Noël Mailloux, Boyd R. McCandless, Fred McKinney, Mortimer M. Meyer, James G. Miller, Robert S. Morrow, C. Roger Myers, Z. A. Piotrowski, Donald A. Ramsdell, M. Gertrude Reiman, Gilbert J. Rich, Anne Roe, Carl R. Rogers, Alan K. Rosenwald, Julian B. Rotter, R. Nevitt Sanford, Helen D. Sargent, Martin Scheerer, Audrey Schumacher, David Shakow, Evelyn Troup, Read D. Tuddenham, Robert I. Watson, David Wechsler, Howard R. White, M. Erik Wright, George Yacorzynski, and Robert A. Young.

In counseling and guidance, Hugh M. Bell, Ralph F. Berdie, John G. Darley, Paul C. Greene, Robert Hoppock, Barbara A. Kirk, Robert H. Mathewson, George Meyer, Harold B. Pepinsky, Milton Schwebel, and Cornelia D. Williams.

In industrial psychology, William C. Biel, Steuart H. Britt, Bernard Covner, Gilbert David, Clifford E. Jurgensen, Raymond A. Katzell, William McGehee, Robert N. McMurry, Walter J. McNamara, Joseph E. Moore, Fred A. Replogle, Phil S. Shurrager, and Neil D. Warren.

The American Board of Examiners in Professional Psychology, Inc. on November 13-14, 1952, administered its fourth written examination to 27 eligible candidates at 17 examining centers in the United States. This written examination included sections of objective questions dealing with the fundamental areas of psychological knowledge and the candidate's own professional field, together with essay questions within the candidate's professional field of specialization.

The Board wishes to acknowledge its indebtedness to the following diplomates who served as proctors: Robert M. Allen, Ralph F. Berdie, Paul

S. Burnham, Blake Crider, Roy M. Dorcus, Philip H. Du Bois, Daniel D. Feder, John V. Gilmore, Paul C. Greene, Margaret L. Ives, Joseph Jastak, Barbara A. Kirk, James W. Layman, Leslie Phillips, Dorothy W. Seago, J. Warren Thiesen, and Philip Zlatchin.

The American Board of Examiners in Professional Psychology, Inc. publicly expresses its appreciation for the services of its diplomates who served as readers of essay questions of candidates taking its fourth written examination in November, 1952. Following are the readers in the field of clinical psychology: Paul Bergman, Molly Harrower, William A. Hunt, Isabelle Kendig, Fred McKinney, C. Roger Myers, Carl R. Rogers, and Howard R. White; in the field of counseling and guidance: Edward S. Bordin, Hilding B. Carlson, Mitchell Dreese, Clifford P. Froehlich, Paul C. Greene, Milton E. Hahn, Nathan Kohn, Jr., Donald Swanson, and Robert Waldrop; and in the field of industrial psychology: Marion A. Bills, Stephen Habbe, Carl Hovland, C. E. Jurgensen, Raymond A. Katzell, and Norman Maier.

VA Department of Medicine and Surgery Clinical Psychology Program Announcements

Rayman Bortner a graduate of the VA Psychology Training Program from Northwestern University has been appointed to the staff at VA Hospital, Augusta, Georgia.

George Charnes a graduate of the VA Psychology Training Program from Western Reserve University has been appointed to the staff at VA Regional Office, Cleveland, Ohio.

Jesse Johnson has resigned as Clinical Psychologist from VA Hospital, Tuskegee, Alabama.

Albert Barrett has resigned from the staff at VA Hospital, Wilmington, Delaware to accept a faculty appointment at University of Florida.

The Spanish Psychological Society (Sociedad Española de Psicología) was founded in Madrid in May, 1952. José Germain is president of the Society and Mariano Yela is the executive secretary. Other members of the Executive Board are Juan José López Ibor, Juan Zaragüeta, José Luis Pinillos, Ricardo Ibarrola, Julian Marias, Gonzalo R. Lafora, and José Mallart.

The International Congress of Psychotechnique, which is meeting in Paris on July 27-August

1, 1953, has announced the plans and organization of its program. The Congress will be organized in four sections: Psychology of Industry, with C. B. Frisby, director of the National Institute of Industrial Psychology, London, as chairman; Clinical Psychology, with R. Nyssen, professor of psychiatry at the University of Brussels, as chairman; Psychology of Education, with R. Zazzo, professor of psychology, University of Paris, as chairman; and Professional Instruction, with Mme. C. Bénassy-Chauffard, secretary-general of the National Institute of Professional Instruction, Paris, as chairman. Anyone planning to attend the Congress should write without delay to Pr. R. Bonnardel, 41 rue Gay-Lussac, Paris 5°, so that papers relating to the Congress may be sent to him. The fee is 3,000 French francs for members of the International Association of Psychotechnique and 5,000 French francs for nonmembers. Payment should be made to Dr. M. Couméto, 101 boulevard Arago, Paris 14°, France.

Psi Chi, national honorary society in psychology, has announced that the 1952 recipient of the Britt Foundation Grant-in-Aid is Percy Black of Fredericton, New Brunswick, for a study of agreement patterns of persons stimulated by different affects. This study grows out of previous work by Mr. Black concerning the relationship of affective states and interpersonal reactions. He is a graduate member of the University of Minnesota chapter of Psi Chi, and is currently working towards his doctorate from Harvard University. The committee which recommended the award was composed of C. E. Hamilton, Alfred University, chairman; Hubert Bonner, Ohio Wesleyan University, and David L. Cole, Occidental College.

Two new chapters of Psi Chi, national honorary society in psychology, were installed during the fall semester. They are the Loyola University (Chicago) chapter, for which Ernest A. Haggard, associate professor at the University of Chicago, was installation officer; and the University of South Carolina chapter, for which M. Kershaw Walsh, chairman of the department of psychology, was installation officer. Frank J. Kobler is faculty adviser of the Loyola University chapter and Erland Nelson is faculty adviser of the University of South Carolina chapter.

Sociological Abstracts is a new cooperative, non-profit quarterly specializing in abstracting socio-

logical and sociologically oriented periodicals. Vol. 1, No. 1, November 1952 and Vol. 2, No. 2, February 1953 are now available. The periodical contains a classification and index. For copies please write to Leo P. Chall, Editor, 218 East 12th Street, New York 3, N. Y., and enclose 50¢ per issue in coins or stamps of low denomination.

The Student Counseling Bureau at the University of Illinois has several thousand used test booklets which they would like to sell at a small fraction of their original cost. The booklets are still usable, and are being offered for sale rather than being discarded, with the thought that other psychologists may wish to buy them for greatly reduced prices. The tests include college aptitude and interest, personality, and high school achievement tests. Inquiries should be addressed to James W. Creaser, Student Counseling Bureau, Undergraduate Division, University of Illinois, Navy Pier, Chicago, Illinois.

The department of psychology, Springfield State Hospital announces a **workshop in the TAT** to be conducted by Leopold Bellak, clinical assistant professor of psychiatry, New York Medical College. The dates are March 20 and 21. Some housing will be available. All inquiries concerning registration fee and other information should be directed to Michael H. P. Finn, Chief Psychologist, Springfield State Hospital, Sykesville, Maryland.

Workshops in the Rorschach method of personality diagnosis and other projective techniques will be conducted next summer at Claremont College by Bruno Klopfer and Evelyn Troup. Workshop A will be held August 3 to 15 and will include introductory seminars as well as intermediate and advanced seminars. Workshop B, August 18 to 28, will consist only of intermediate and advanced seminars. To apply for admission write to Dr. Bruno Klopfer, 480 Redwood Drive, Pasadena 2, California, before July 1, 1953.

Corrections and additions to the article on "Training Facilities and Financial Assistance for Graduate Students in Psychology" in the January 1953 American Psychologist

The departments of psychology at the Ohio State University, University of Washington, and Yale University should have been listed as

approved by the APA's Education and Training Board for doctoral training in clinical psychology.

Bowling Green State University offers general preparation for the PhD degree.

Brandeis University stipends. Brandeis University, Waltham 54, Massachusetts. Apply for admission to Dr. Abraham H. Maslow, Chairman of the Committee on Psychology, Graduate School of Arts and Sciences. Tuition \$600. GRE and MAT required. A number of graduate scholarships, graduate fellowships and teaching assistantships will be awarded for the academic year 1953-1954. Graduate scholarships—full remission of tuition and fees plus stipends up to \$600. Graduate fellowships and teaching assistantships—full remission of tuition and fees plus stipends up to \$1,400. These awards will be made on a competitive basis to men and women candidates of outstanding academic potential. Under exceptional circumstances larger sums may be awarded. Enrollment in the Graduate School will be limited to assure students of the close personal guidance and supervision necessary in joint scholarship.

Cornell University, School of Education. One vacancy: stipend \$1,800, student must pay his own tuition (\$300). Responsibilities are in testing and research work in the University Testing and Service Bureau. One vacancy: stipend \$1,600, tuition exempt. Responsibilities are in the teaching of general psychology. Two vacancies: stipend, \$900, tuition exempt. Stipend \$1,800, student must pay his own tuition (\$300). Responsibilities are in the University Reading Laboratory working with students in the reading improvement course. Apply by April 1 to Dr. M. D. Glock, 311 Stone Hall, Cornell University.

Fordham University. The School of Educational Psychology, Measurements and Guidance, offers graduate work for the doctor's and master's degrees in educational psychology, counseling, and tests and measurements. From 1949 to 1952 this division granted 19 doctoral degrees and 105 master's degrees. Assistantships and scholarships are available each year. Write to Dr. William A. Kelly,

Chairman, Division of Educational Psychology, Measurements and Guidance, School of Education, Fordham University, New York 7, N. Y.

University of Manitoba has training in educational psychology under the Faculty of Education.

University of Minnesota. *Institute of Child Welfare*: Training program to master's and doctor's degree level in child and developmental psychology. During the period 1949-52, 31 master's and 4 doctoral degrees were granted. Nine teaching or research assistantships; stipend, \$855-1,283; new students eligible. Apply to Dr. John E. Anderson, Director, Institute of Child Welfare.

Educational Psychology: During the period 1949-52, 128 master's and 12 doctoral degrees were granted. Fifteen teaching, counseling, and research assistantships; 10-12 hours' work; stipend, \$641-1,283; new students eligible. Apply to Dean Walter W. Cook. Various half- and full-time counseling assistantships in Student Counseling Bureau, College of Science, Literature, and the Arts, General College, and College of Education. Apply to Director of Counseling Bureau or Dean of the College.

Wayne University. There are three departments at Wayne University offering graduate work in psychology: the College of Liberal Arts, School of Business Administration, and College of Education. The information in the article was submitted by the department in the College of Liberal Arts. The following information has been submitted by the department in the College of Education. The administrative officer is Dr. Gertha Williams. For the academic years 1949-52, 156 master's degrees were awarded. Master's degree training is offered in the fields of child and developmental, educational, and clinical psychology. Tuition is \$74 a semester; nonresident tuition is \$100 a semester. The Miller Analogies Test is required for admission to the clinical program. Three teaching assistantships are available with stipends of \$1,075; 20 hours' work per week; new students eligible. Apply to Dr. Gertha Williams, College of Education, Wayne University, Detroit 1, Michigan.

1953 Annual Meeting

The 1953 APA meeting was scheduled to be held at Michigan State College. Because of recent turns in the negotiations with Michigan State, the Council of Representatives recently voted to hold the meeting elsewhere if suitable arrangements could be made. The Council voted a preference for Cleveland. Adequate facilities are available there. The 1953 meetings will be held, then, in Cleveland on September 4-9. The May issue of the *American Psychologist* will carry information about local arrangements.

Convention Calendar

- American Psychological Association:** September 4-9, 1953; Cleveland, Ohio
For information write to:
Dr. Fillmore H. Sanford
1333 Sixteenth Street N. W.
Washington 6, D. C.
- American Personnel and Guidance Association:** March 29-April 2, 1953; Chicago, Illinois
For information write to:
Executive Secretary
American Personnel and Guidance Association
1534 "O" Street N. W.
Washington 5, D. C.
- Southern Society for Philosophy and Psychology:** April 3-4, 1953; Austin, Texas
For information write to:
Dr. Oliver L. Lacey
Department of Psychology
University of Alabama
University, Alabama
- Rocky Mountain Branch of APA:** April 3-4, 1953; Albuquerque, New Mexico
For information write to:
Dr. Lawrence S. Rogers
1046 Madison St.
Denver, Colorado
- National Association of Student Personnel Administrators:** April 6-9, 1953; East Lansing, Michigan
For information write to:
Dean Tom King
Michigan State College
East Lansing, Michigan
- International Council for Exceptional Children:** April 8-11, 1953; Boston, Massachusetts
For information write to:
Harley Z. Wooden, Secretary
1201 Sixteenth Street N. W.
Washington 6, D. C.
- Illinois Psychological Association:** April 11, 1953; Chicago, Illinois
For information write to:
Professor George S. Speer
Institute for Psychological Services
3329 South Federal Street
Chicago 16, Illinois
- American Psychosomatic Society:** April 18-19, 1953; Atlantic City, New Jersey
For information write to:
Miss Joan K. Erpf
American Psychosomatic Society
551 Madison Avenue
New York 22, New York
- Florida Psychological Association:** April 23-25, 1953; Miami, Florida
For information write to:
Dr. Theron Alexander
Department of Psychology
Florida State University
Tallahassee, Florida
- Eastern Psychological Association:** April 24-25, 1953; Boston, Massachusetts
For information write to:
Dr. G. Gorham Lane
Department of Psychology
University of Delaware
Newark, Delaware
- West Virginia Psychological Association:** April 24-25, 1953; Charleston, W. Va.
For information write to:
Dr. Frieda K. Merry,
Morris Harvey College
Charleston, West Virginia
- Midwestern Psychological Association:** May 1-2, 1953; Chicago, Illinois
For information write to:
Dr. Lee J. Cronbach
Bureau of Research and Service
University of Illinois
1007 1/2 South Wright Street
Champaign, Illinois
- American Society of Group Psychotherapy and Psychodrama:** May 4-5, 1953; Los Angeles, California
For information write to:
Dr. E. F. Borgatta
Laboratory of Social Relations
Harvard University
Cambridge 38, Massachusetts
- American Psychiatric Association:** May 4-9, 1953; Los Angeles, California
For information write to:
Dr. Cullen Ward Irish
1930 Wiltshire Boulevard
Los Angeles 5, California
- Acoustical Society of America:** May 7-9, 1953; Philadelphia, Pennsylvania
For information write to:
Dr. Harry F. Olson
RCA Laboratories
Princeton, New Jersey
- California State Psychological Association:** May 9, 1953; Los Angeles, California
For information write to:
Dr. James F. T. Bugental
Department of Psychology
University of California
Los Angeles 24, California
- Pennsylvania Psychological Association:** May 9, 1953; Harrisburg, Pennsylvania
For information write to:
Dr. William U. Snyder
Department of Psychology
Pennsylvania State College
State College, Pennsylvania
- American Association on Mental Deficiency:** May 12-16, 1953; Los Angeles, California
For information write to:
Dr. Neil A. Dayton
P. O. Box 96
Willimantic, Connecticut
- Canadian Psychological Association:** May 28-30, 1953; Kingston, Ontario, Canada
For information write to:
Dr. G. A. Ferguson, Secretary-Treasurer
3544 Peel Street
Montreal, P. Q., Canada
- American Psychopathological Association:** June 5-6, 1953; New York City
For information write to:
Dr. Donald M. Hamilton
121 Westchester Avenue
White Plains, New York
- Western Psychological Association:** June 18-20, 1953; Seattle, Washington
For information write to:
Dr. Richard Kilby
Department of Psychology
San Jose State College
San Jose, California
- Association Internationale de Psychotechnique:** July 27-August 1, 1953; Paris
For information write to:
Pr. R. Bonnardel
41, rue Gay-Lussac
Paris 5*, France

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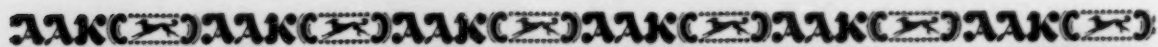
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